

Lattice Field Theory: past, present and future.

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Abstract

This letter is in response to a recent review by DeTar and Gottlieb about lattice QCD that has recently appeared in Physics Today. It also is partially motivated by a separate review written by DeGrand. My basic point is that one should be more responsible when presenting numerical results as coming from *ab initio* calculations. In turn, this leads me to the suggestion that in lattice field theory one should go back to computational support going directly to small groups, including groups containing only a single researcher, rather than concentrating most of the funds into the hands of few large collaborations.

DeTar and Gottlieb's article [1] misleads readers from outside lattice field theory about its past, present and future. At best, it may present a consensus in the collaboration the authors are part of, known as MILC.

The terms "lattice field theory (LFT)", "lattice gauge theory (LGT)" and "lattice QCD (LQCD)" are often used interchangeably. I shall follow this practice; the more accurate term will be clear from the context. Also, I have not put in references to any papers except the ones I quote from *verbatim*.

After an overview of the history of LFT the authors of [1] state that "the most important theoretical advance in recent years" was that of "improvement". I disagree with this qualification. In their historical overview the authors miss too many of the field's truly remarkable achievements. The description below briefly mentions *some* of these achievements and is intended to show that "improvement" does not measure up.

The renormalization group (RG), a conceptual organizing principle of all field theories, has been first concretely formulated in LFT. Nowadays we have beautiful examples of intricate flow patterns between different fixed points of continuum field theories showing that these concepts actually work to full extent. As a concrete example of the value of numerical LFT let me mention one non-gauge result of relevance to particle physics: triviality is indeed a property of scalar field theories and implies a non-perturbative bound on the Higgs mass of the order of 700 *GeV*. Another conceptual idea which started its life in LFT is the concept of duality. Two dual field theories typically have different fields, different symmetries, but, if the coupling in one theory is set to the inverse

coupling in the second theory they become equivalent descriptions of the same physical entity. This is again a concept that has been dramatically validated by marvelous modern examples. In the context of duality, monopoles have played a central role: again something LFT can be proud of (including some of today's members of MILC). Moving on, we recall the conceptual pictures of confinement and finite temperature deconfinement. The first decade of LFT has been extremely productive and we see that it has had a long lasting impact on theoretical particle physics and field theory. Abstract concepts have successfully been buttressed by concrete calculations and accurate and reliable numbers were produced by simulations of bosonic degrees of freedom. One could call this the “bosonic era of LFT” and it is an illustrious one.

The inclusion of fermions, a much needed step beyond the bosonic era, has preoccupied a large fraction of the community, in various forms. Fermions had a conceptual defect in their original formulation by Wilson. A different and ingenious formulation, by Kogut and Susskind, had defects of a similar kind. For twenty years it was believed that continuum flavored chiral symmetries could not be reproduced on the lattice, although they seemed to have nothing fundamental to do with continuum ultraviolet physics. (Chiral symmetries are important in particle physics because they provide a key mechanism for naturally small masses.) During the last decade, finally, the problem of lattice chirality has been solved. A large number of papers and their citation counts attest to the interest this development was received with. Thus, without making claims about how this measures up to the bosonic era, this was an important theoretical advance in recent years.

On the numerical front, progress on fermion systems was held in check by computer technology. In the US, a very major fraction of computer resources was given to few large collaborations, and MILC is the oldest among these. Progress was made at essentially the rate that computer power got cheaper. On the way, a significant physical step was the formulation of the valence approximation and establishing its surprising numerical accuracy when compared to experiment. At the more technical level, probably the most significant step was the discovery of an algorithm that would be able to take us beyond the valence approximation, to truly *ab initio* numerical QCD. Neither of these two steps originated from MILC, although MILC made contributions at later stages. Both the surprises surrounding the valence approximation, and the algorithms making it feasible to go beyond it, would qualify as important advances, whether “theoretical” or not is a matter of semantics.

Since the initial formulation of LFT, and the associated RG ideas, it was clear that the approach to continuum could be sped up by fine-tuning the lattice action. This is the improvement technique, and it is very important in practice, but it does not have the theoretical novelty any of the above achievements have.

The newest results the article describes go beyond the valence approximation and use clever improvement tricks. As a result, if we ignore one less appealing trick that is being used, we could say that improvement has made the corrections to the continuum limit vanish as $\frac{a^2}{[\log[a\Lambda_{\text{QCD}}]]}$ rather than a^2 , where a is the lattice

spacing of the grid. The improvement trick is certainly valid research, but not a breakthrough and certainly not the “most important theoretical advance” of recent years.

However, the real problem, and one that the article hides from the unsuspecting reader, is that these recent calculations are not *ab initio* because of “the less appealing trick” used to include sea quarks. It is true that ordinary effective field theory (EFT) logic would support the use of simulations based on Kogut-Susskind fermions for gauge theories with four-fold degenerate flavors. In other words, if each flavor were to come in four identical “tastes”, one could make a case, at least on the level of EFT logic, that, so long as asymptotic freedom is preserved, we can, in principle, carry out an *ab initio* numerical calculation for observables in this field theory. However, the simulations are claimed to describe QCD, and there is no such degeneracy in QCD. For this reason, the simulations described employ a trick in which the contributions of sea quarks are reduced by a factor of four. However, even a generous application of EFT lore, defending this procedure, has never been proposed. If we did not carry out this artificial suppression by one quarter, the effective lagrangian would have terms that violate taste equivalence at order $\frac{a^2}{|\log[a\Lambda_{\text{QCD}}]|}$. There is no place I know of that sketches a derivation of an EFT description for what is actually being simulated, inclusive of the artificial factor of one quarter suppression of sea quark contributions.

Moreover, EFT logic is usually applied by looking only at the local properties of the theory and can go wrong if there is an important global property that has not been properly taken into account. In our case there would be reason to be cautious even if an EFT argument supporting the artificial one quarter suppression were presented: Apparently, Kogut - Susskind fermions can be viewed as Grassmann valued antisymmetric forms and, as such, could be defined on a lattice approximating a manifold, like CP^2 , which does not admit a spin structure and, therefore, cannot accommodate four non-interacting (in a fixed background) degenerate Dirac fermions. It is unlikely that the fermion determinant has an acceptable fourth root in this case. Typically, EFT is applied without paying attention to global topology, so, at least in this example, could easily have led us astray.

Contrary to the impression the article [1] conveys, a right way to full QCD is known, based on exact lattice chirality. Again, computational cost holds us back, but there is little doubt that more years will bring us the power we need to do the calculations right. It seems now, and, if one is so inclined, the recent result reviewed in the article [1] might be taken as supportive of this hypothesis, that during the next ten years, or so, we shall be able to present to the rest of the particle physics community some reasonably accurate numbers that were obtained directly from the QCD lagrangian, with no other assumptions. But, it is wrong to present the two methods of including fermions, one based on Kogut-Susskind fermions and the other based on lattice fermions with exact chirality, as on equal theoretical footing. Nevertheless, [1] mentions unspecified “cross checks” between the two fermion methods, as if they were conceptually

equivalent. The truth is that once one can do simulations with truly chiral fermions, all QCD work based on Kogut-Susskind fermions can be forgotten.

I disagree with the authors that LQCD has matured; rather, its practitioners have, and their relentless pursuit of computer resources seems to have drained some of them of the selfdiscipline required when presenting ones results to the rest of the particle physics community. Statements like “the most interesting lattice calculations with dynamical simulations are the ones done by the MILC collaboration” [2], written by another member of MILC, are misleading even if later on in the paper one finds a technical discussion of caveats [2]. In the same paper we read: “Finally, because they are so big, lattice projects have a high profile. They cannot be allowed to fail.” It is left unclear whether this speaks for the reliability of the results coming from these projects, or, against it.

Unlike experiments in nuclear or particle physics, lattice projects do not require large numbers of people, so do not *have* to be big in terms of personnel. I think that only if it again becomes acceptable that some projects fail, and projects are not forced to be so big, does LFT stand a chance to restore some of the status of reliability and respect it used to enjoy in theoretical physics. We should rethink the policy that concentrates almost all of the computing power in the hands of few large collaborations. The past has taught us that this has a tendency to stifle individual thinking and imaginative risk taking. People who broke the mold sometimes were penalized without scientific justification and ended up leaving LFT. Now is a good time to rethink this policy because an alternative exists: small but reasonably effective commodity clusters have reached prices that are reasonable for small groups and even individual researchers. Money would be better spent if, say, one half of the resources allocated to LFT were distributed to small research groups, or even single researchers, earmarked for purchasing compute-clusters. No science coming from the large collaborations would be lost if their funds were restricted to the remaining half of the entire computation budget of LFT.

References

- [1] Carleton DeTar and Steven Gottlieb, Physics Today, February 2004, 45.
- [2] Thomas DeGrand, hep-ph/0312241v1.